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Abstract

This document, prepared for a symposium on preschool compensatory programs, makes preliminary comments on the difficulty of program assessment and tight experimental design, on the necessity of in-depth involvement, and on the need for vigilance in maintaining an experimental condition. A number of studies in preschool intervention are reviewed and evaluated. Among these are the Skeels (1966) 21-year followup study on institutionalized, retarded children, the Klaus and Gray study (1968-69) involving low income Negroes in the upper South in a summer and home-visit followup program, and Weikart's study (1967) utilizing 2-1/2 hour morning sessions and afternoon home visits. Two curriculum comparison studies (Weikart, 1969 and Karnes, 1969) and a study of comparative kindergarten conditions for rural children are also discussed. (MH)

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SELECTED LONGITUDINAL STUDIES OF COMPENSATORY EDUCATION--A LOOK FROM THE INSIDE*

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These days I seem constantly to find myself considered a pioneer in the field, and the author, along with Rupert Klaus, of a so-called prototype study of early intervention. This always surprises me, since the Early Training Project, the "prototype," began its first assembled work with children just seven years ago. It certainly points out, however, that the widespread concern with programs of early intervention is of recent origin, and must have grown rapidly.

Many of the difficulties in adequate national assessments of such programs relate to the extremely rapid growth. Both Project Head Start and Follow Through were initiated with great haste. Desirable as this may be from the service aspects, it makes any evaluation of quality difficult. Head Start programs, for example, have been shaped to a considerable extent at the local level. Thus, they have been wildly heterogeneous--some excellent programs and some as poor as others have been excellent. If such programs are thrown together, inevitably the bad cancels out the good, when assessment time rolls around.

My presentation this afternoon will take a look at a few relatively well defined and controlled studies of early intervention, ones which are far enough along to give us a picture of results over time. It is hoped that this will add another dimension to our answer to the thorny question of how to improve educability of young children from low income homes.

As a "pioneer" in the field, perhaps I may be indulged to point out some of the things we have learned the hard way at Peabody, as we have tried to design, implement, and evaluate experimental programs.

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First of all, there is the issue of the amount of work it takes to get a measurable effect in terms of the improved functioning. In what are now several studies of intervention programs with young children at Peabody we have found sustained but modest gains. Yet our programs would by many people be considered massive ones, lasting a considerable period of time, and involving a high level of adult-child contacts and extremely detailed planning. This has made us rather cautious about programs that report a high level of gain after limited amount of intervention.

Secondly, we have learned the hard way that one must keep extremely close to the experimental treatments, if one is attempting an approach at all different from the accustomed practices of those who are implementing the treatments. I'd like to think of the lack of this as "innovative entropy"--a drift over time to the more familiar practices of the implementer of the program. This is typical of programs where teachers are brought in for a two to four weeks' training period, and then pretty much turned loose. Unless they are thoroughly practiced in the new method before they begin, day by day they will drift back to the older and often easier ways.

A third problem in field studies is maintaining over time anything worthy of the name of experimental design. There are many aspects of this problem. There is the high mobility of the sample which one is studying. Kuno Beller told me recently that the over 200 children, with whom he started, six years ago at age four, are now in 80 different schools. There is the problem of the equivalence of measures over time, if one is studying gain scores. The younger the child the more acute are the problems of changing test content. There are continued field pressures to move away from one's experimental design. In an area of high public visibility people are eager for results --now. Maintaining an uncontaminated control group is next to impossible. As one of my colleagues says, "We might as well face it that one man's experimental group is going to end up as another man's control group."

The final item on this "sadder but wiser" list is the supreme importance of the child's total milieu before, during, and after intervention. But more of this presently.

With all these factors making it difficult to conduct adequate intervention programs over time, it is hardly surprising that the number of well-designed, implemented, and carefully documented studies are few in number. I have attempted to select five studies for a fairly close look at some of the aspects which should be helpful to us as we tend to bring some order into the rather chaotic findings on early intervention programs. With some of these studies you probably already are familiar. I have tried to include the most recent data from them. Thus, I hope you will forgive me if I go over some of their more salient points as they relate to our major concern.

The first of these studies is the real grandfather of the whole lot (and probably the most familiar)--Harold Skeels' (1966) study which represents a twenty-one year

follow-up of the early studies by Skeels and Dye (1939) and by Skodak (1939). You will probably recall that Skeels' experimental group consisted of 13 children in an orphanage, with an average age of 19 months and an I.Q. mean of 64 at the beginning of intervention. These were youngsters whose development was so delayed that no adoptive placement had been made for them. The treatment consisted of placing these children in an institution for the mentally retarded under the care of some of the older female inmates. Twenty years ago this placement was looked upon with derision by the psychological public. Hindsight today, however, would suggest certain elements of this placement conducive to the child's general development. The orphanage from which they came was overcrowded, with limited resources and staff. In the ward placement in the institution for the mentally retarded, a patient or attendant took over the role of mother-surrogate for the child. She spent large amounts of time playing, talking, and training the child. The living quarters in the institution were spacious; there was an outdoor playground with appropriate materials. Skeels reports that the inmates took great pride in "their" children, and indeed developed some competition as to whose child could learn the most. The children attended nursery school and kindergarten, and also were exposed to other sorts of enrichment programs.

Skeels' contrast group was one of 12 youngsters considered normal in mental development and placeable, although for various reasons they had not been placed at the beginning of the study. The mean I.Q. was 87, and the chronological age was seven months at the start. After about two years, the experimental group had gained 28 I.Q. points, and the contrast group had lost almost an equal amount.

In the first follow-up study, 11 of the 13 experimental children had been placed in adoptive homes and had retained their earlier gains in intelligence.

The most striking results are the ones after 21 years. Here Skeels found that the two groups had continued to maintain their diverging patterns as they had moved into adulthood. The median grade completed in school was 12 for the experimental group, while it was third grade for the control group. All members of the experimental group were self-supporting; five of the contrast group remained in wards of institutions. Incomes were markedly different. One could not call the Skeels study a neat, clean, experimental design. On the other hand, its results are so striking that it is a landmark in the field. Two things occur to me as particularly important. One, the placement in the institution for the mentally retarded appeared to have carried with it two of the most desirable aspects of the early intervention programs--a high adult-to-child ratio in a warm, and presumably consistent, environment, and a high stimulus potential in the environment as compared to their earlier situation. Above all, there was continuous intervention over time--it did not cease when the child was placed, but rather continued. And at the same time another kind of intervention continued for the contrast group, one which had all the adverse effects of the early environment.

The next study I would like to discuss is the one that Rupert Klaus and I initiated in 1961--the Early Training Project (Klaus and Gray, 1968; Gray and Klaus, 1969). We now have analyzed data on the subjects through the fourth grade--a long way from the adulthood of Skeels' study, but still along enough to have some idea of what can happen over time.

The children with whom we worked were from low income Negro homes in the upper South. The children were randomized into three groups. One group entered our intervention program three summers prior to public school entrance into the fourth grade, one experimental group two summers prior to such an entrance; the third group had no intervention. A fourth group was included in the design, a similar group from a town 60 miles distant. Our intervention consisted of an assembled program during 10 weeks of the summer, followed by nine months of weekly home visits from a specially trained worker. These visits had as their primary purpose an attempt to involve the mother actively in sustaining and increasing the gains the child had made during the summer months. The intervention program was constructed as carefully as we knew how, around variables relating to aptitudes conducive to achievement. There was a ratio of one adult for every four children. In 1964 all of these children moved into the first grade--the three local groups into the same school.

During the seven years of this study we have been particularly fortunate in that we have enjoyed an absence of field pressures to change our experimental design or to draw premature conclusions. There has been little mobility in the group. Out of the three local groups we have been able to maintain a constant testing schedule for 56 of the original 61 children through the seven years. We have had slightly more attrition in the distal control group.

Our situation and experimental design has made possible some interesting comparisons. On the test of intelligence we used consistently, the Binet, the experimental group has remained significantly superior at the .05 level to the control groups. Up until the age of school entrance, the experimental group showed a modest gain accelerated beyond what had been anticipated; the control group showed a slight decline. In first grade all groups improved, and then over time, in a parallel fashion across four groups, there has been a slight decline, which we interpret as probably related to the massive impact of the environment and the school situation, which could be categorized at best as mediocre. Significant differences remained on the Illinois Test of Psycholinguistic Abilities through first grade, and through the second grade on the Peabody Picture Vocabulary Test.

On the two tests of school achievement used, the Metropolitan and the Stanford, the experimental children have shown themselves to be slightly, but consistently superior, to the experimental children. The differences reached the level of significance in from one third to one half of the subtests during the first two years of the schooling. By the end of fourth grade, however, the differences were no longer significant.

You can see now what I meant earlier about saying how hard it is to get a significant effect. Yet to us the remarkable thing is that with all the impact of home, school, and community, the differences on intelligence are still holding up, and that consistent trends remain on the other measures.

I should like to mention two other findings from our study, relevant to planning adequate intervention programs. One is our analysis of what we can call horizontal diffusion, the spread of effect from children and parents in experimental groups to other children and parents living in close proximity to them. The Negro group in our local city lives close together. There is a great deal of intermarrying among this group. We checked out consistent contacts between the experimental and control families over time. Here we found that there was only one child in the control group who had no contact with the children or families in the experimental groups. Where an intervention program is seen as highly desirable by the community, as this one was, an ideal setting is provided for spill-over effects. In terms of first and second grade performance, the comparison of the local and distal control groups does make some sort of analysis possible of the effects of the contact over time. The findings on the product of diffusion are at least suggestive of some measurable effect of this horizontal diffusion.

Even more interesting to us has been what we have called vertical diffusion, the spread of effect from older to younger siblings. Here we have tested twice, with the Binet, the younger siblings of the four groups of children. The younger siblings who were old enough to test in 1964 were tested again in 1966, and a new crop was picked up. The children closer in age to the experimental children were significantly superior on both the 1964 and the 1966 testings to the younger siblings from the control groups. Apparently, in terms of intervention techniques taught the mother there is more spread of effect to the children closest in age to those for whom the technique was designed.

The next study I would like to discuss is that of David Weikart and his fellow researchers in the Ypsilanti, Michigan, schools (Weikart, 1967). Here I would like to go into some of the findings of his Perry Preschool Project, and then a recent study of Weikart which violates our criterion of long-term longitudinal data, but which is highly relevant to our general concern. The Perry Preschool Project began in 1962; in Weikart's 1967 report, which contains the latest data available on the original studies, four groups, or "waves", have been through two years each of the intervention program. The program has changed somewhat in its nature over time. It began originally as a program of "verbal bombardment" but moved over the years to an approach somewhat more Piagetian. Weikart groups were three- and four-year-old children from culturally deprived families, who tested in the range of "educable mentally retarded." The mean initial Binet I.Q. of the so-called waves over the four years varied from 78 and 80. The school based program consisted of a 2-1/2 hour morning class for the youngsters. There was in addition a home-based afternoon program.

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The teachers of the morning program visited in the home of each youngster one afternoon per week. The teacher brought along equipment from the school and attempted to extend into the child's home teaching on a one-to-one basis. Originally the mothers' role was that of being present and of observing. Over time this has apparently moved into much more active interaction with the mother herself. Weikart reports findings upon the Binet, the PPVT, and the Leiter International Scale. At the end of first grade, he reports findings upon the California Achievement Test, for his first wave only. On the Binet the first two groups showed an initial superiority after completion of one year in preschool as compared to a control group, but after that time differences still existed, but failed of significance. With the third wave, however, the superiority was maintained through the second year of the preschool. Findings tended to be somewhat similar on the PPVT and the Leiter. Initial scores are lower, but the same general pattern of progress is shown. It is particularly noteworthy, to me at least, that the first wave group, although not significantly different from the control on the Binet, the PPVT, or the Leiter at the completion of kindergarten, did show themselves superior on all subtests of the Gates Reading Test and the California Achievement Test at the end of first grade. It will be interesting to see data from the later groups, particularly for the third, since it did not show converging of the experimental and control scores during the second year of preschool.

The second study by Weikart (1969) which I would like to describe briefly is one that compares three curricula for young children, or such was the initial purpose. The study compared three curricula thought to be of possible value for the disadvantaged: (1) A "unit-based curriculum" which emphasized the socio-emotional development goals of the somewhat typical nursery school program. An effort was made to introduce children to the wider environment, to pay close attention to individual, social, and emotional needs and to allow a high degree of permissiveness in classroom operation. (2) A "cognitively oriented curriculum" following the one developed in the Perry Preschool Project over time. Weikart describes this as based on methods of "verbal bombardment," and socio-dramatic play and showing principles derived from Piaget's theory of intellectual development. (3) A language training curriculum emphasizing acquisition of academic skills. This last was the Bereiter-Engelmann approach. There was one group of three-year-olds and another group of four-year-olds. In addition, there was an appropriate control group. Although the number of cases is small, there was a striking consistency in the two age groups. For the four-year-olds, gains for the three curriculum groups, varied between 18 and 24 I.Q. points on the Binet, while the contrast group gain was only three points. With the younger children, the gains were between 28 and 30 with a 0.4 gain for the contrast group. These findings are the more impressive when one views the fact that David Weikart is a highly experienced and competent worker in the field. One contrast group made virtually no gain and the other made what would be expected as a minimum gain on the basis of test-retest. The other striking finding, of course, is the lack of difference among the three curriculum groups. Results are basically similar on the PPVT and the Leiter. Findings such as these make one

wonder as to whether our current search for specific curricula for the disadvantaged children may be taking us in the wrong direction. We shall return to this presently.

One thing we must keep in mind is that Weikart and his fellow workers are very careful in their intervention techniques. This is not simply the failure to maintain an appropriate differentiation among the three curricula nor is it a case of the so-called Rosenthal effect. If the latter were operative, surely the Perry project curriculum model would have come out ahead.

Merle Karnes (1969) has, during the year, reported the findings on a three-year study comparing five general approaches to curricula for young, deprived children. One of these was the so-called Traditional nursery school, aimed to promote personal social moda and general language development of the children. A second was the so-called Community Integrated program, which provided a traditional nursery school, but was operated by community groups and primarily came from middle and upper-class Caucasian parents. The third program was a Montessori-type program which met Montessori standards. The fourth program was the "Ameliorative" program which emphasized verbalization in connections with manipulating concrete material. Each classroom was divided into three subgroups, and structured learning experiences were devoted to math concepts, language arts and reading readiness, and social science studies. The last program was the Direct Instruction program, which was basically a Bereiter-Engelmann-type program. This program emphasized intensive oral drill, and verbal and logical patterns, with a general instructional strategy of learning a rule which was then followed by application. Arithmetic emphasized a "science of counting" approach. The children were also taught to read with a modified Initial Teaching Alphabet. The subjects for Karnes' study came from economically depressed areas of Champaign-Urbana, Illinois. The mean I.Q. of the groups ranged from 93 to 96. The major intervention, which began at four years, lasted one year for the Traditional, Community Integrated, and Montessori groups. The Ameliorative group, which along with the previous three, entered kindergarten at age five, received one hour a day instruction in addition during the kindergarten year. The Direct Instruction group did not attend the regular kindergarten, but instead continued in the Bereiter-Engelmann program. For first grade, all children attended the public schools of the city.

Data through the pre-school experience and through kindergarten are available on all five groups. For the first grade, data are available only on the Traditional, the Ameliorative, and the Direct Instruction groups. Karnes made intensive analyses of results upon the Binet, the ITPA, and when appropriate, tests of reading readiness and school achievement tests. At the end of the year of intervention, gains in the Traditional, Community Integrated, and Montessori groups were approximately the same, from five to eight points. In the Ameliorative and Direct Instruction groups, gains were 13 and 14 points. During the second year in which the children entered regular kindergarten, with the sole exception of the Direct Instruction group, no gains were shown, not quite maintaining the level of the previous year. The Direct Instruction

group, which you will remember, continued in the Bereiter-Engelmann program, showed an additional gain of six points. The Traditional group, which had shown little gain, maintained its preschool level during first grade. Both the Ameliorative and Direct Instruction groups fell back somewhat, although the losses were slight.

Results on the ITPA are relatively complex. Briefly, the Direct Instruction and Ameliorative groups showed a modest amount of gain on the IPTA, while in active intervention. They tended to drop back somewhat, however, in first grade, so the three groups compared at this time were not significantly different on the IPTA. First grade achievement tests, on the California, showed the Ameliorative and Direct Instruction groups to be superior, with performance somewhat in advance of the actual expected mean score on the basis of grade placement. It is of interest to note that on the basis of actual skill performance the Ameliorative and the Direct Instruction groups performed in a similar fashion, both showing superior performance. Merle Karnes' interpretation of this is somewhat similar to David Weikart's approach to the functionally equivalent curriculum.

Hodges, McCandless, and Spicker (1967) have labeled their study: The development and evaluation of a diagnostically based curriculum for preschool psycho-socially deprived children. I have selected it for examination because of the careful design. There were three groups of children in successive years beginning during the academic year 1964 to 1965. These came from rural and semi-rural southern Indiana. Comparisons were made across three groups: the "diagnostic" kindergarten, a regular kindergarten, and a no kindergarten group. The diagnostically based curriculum attempted to individualize instruction in terms of the children's scores on the language tests used, the ITPA, and the PPVT, upon the Oseretsky Test of Motor Development. Several other attempts were made to assess auditory and visual perception, articulation, and the like. The endeavor to base a curriculum diagnostically is a praiseworthy one. There is, unfortunately, the perennial problem of effective differential diagnosis--there must be effective differential treatments to follow upon the diagnosis. On the Binet, the ITPA, and the PPVT, which were used as pre- and post-test measures, the group with the most gain was the diagnostically based curriculum, followed by the regular kindergarten, which in turn was superior to the control. Follow-up data are reported on the first two groups of children through the second grade, and on the third group through the first grade. Through the second grade the two groups of intervention children--diagnostically-based and regular kindergarten--maintained their gains. By the end of second grade, however, the control children had caught up enough so that differences were no longer significant between the treatment groups and the control groups. Findings tended to be similar on the PPVT and on the ITPA. On the California Achievement Test, used during the first two grades, significant differences did not appear. Spicker has recently given me some follow-up information through the next year of schooling, where particularly interesting findings relate to school failure or placement in special classes. It should be remembered that the children in this study started from a relatively low baseline, with a mean I.Q. of 75. Failure and special classroom placement has been typical of the control groups. The experimental groups

have tended to remain in the regular classroom with approximately two-thirds of them being described as in this sense, successful. The one of the three groups with the best record is, interestingly enough, the second group which Spiker describes as having had the most directive intervention, probably most like the typical first grade in content. He thinks this means, if one has no control over the follow-on experiences of the child, that preschool intervention has to be close to the actual school situation if it is to result in more effective school performance.

I have not mentioned many studies, that from reports seem to be well designed and executed, but which are either not fully reported or are without sufficient time so far to judge results. Furthermore, in those few studies I have described briefly there is a great variation in the kinds of programs and of children, in the length of intervention, the techniques used, and the length and frequency of follow-up testing. Even so, one may try to pull out some relevant dimensions or findings from these studies and others that may be helpful in planning future intervention programs with young children.

Let us start with the simpler variables. First, we may look at the age of intervention. It would come as no surprise that the programs that have intervened earlier have tended to show the greatest gain, at least temporarily. Studies beginning with four-year-olds show more gain than those beginning with five-year-olds. See for example the Weikart data, and our findings on the Early Training Project where the first experimental group tended to show greater gain during the intervention period than the second group, which began a year later.

The initial ability level is another factor. In general, the programs that begin with children with I.Q.'s in the high 70's and the low 80's at initial testing have tended, at least on intelligence tests, to show more gain than those with higher I.Q.'s at initial testing, or with lower I.Q.'s. The Hodges, et.al. children, for example, showed less gain than the Weikart children. The Weikart children have shown slightly more gain than the Early Training Project children, particularly in the Weikart's later waves. There may be a regression phenomenon here, but there is also a suggestion that programs currently planned to offset the environmental deficits of the disadvantaged seem to work best with children with a moderate but not great amount of retardation.

Another finding which is less clearly documented in these studies but is a prominent factor in the Skeels study, and in some of our findings in the Early Training Project, is that of the degree of change in the on-going milieu. The Skeels study is, of course, the most dramatic. In the Early Training Project we have been able to compare eight youngsters who moved into previously all white schools with comparable youngsters who remained in their original school which was nearly all Negro. Since the numbers have been small we do not take these findings too seriously. Still, it has been interesting that the youngsters who moved have tended over a three-year period to show,

on the Metropolitan Achievement Test, approximately three years of gain--the progress of so-called normal children. Non-movers, when matched, as best one can, on a general rating of the home situation, parental aspirations for the child, and first grade achievement level of the child, have made only two years' progress in the three academic years.

Another finding of some interest is that of what might be called delayed effects--ones that do not emerge immediately. It will be remembered in the Perry Project experimental and control groups of the first wave were not significantly different on measures of intelligence at the end of the second year of intervention and at the end of kindergarten. These experimental youngsters, however, did show themselves superior on every subtest of the Gates Reading Test and the California Achievement Test at the end of first grade. Some of the Head Start follow-up studies suggest the same thing. For example, Hyman and Kliman (1967) have reported a study in which they found that Head Start children who entered a middle class public school sustained their gains over non-Head Starters, while similar Head Start children who went to a slum school did not maintain their gain. In the Early Training Project we found more significant differences between experimentals and controls on achievement tests at the end of second grade than we did at the first. Our own impression, which may apply possibly to the other programs as well, is that the experiences of the experimental children tended to define somewhat the curriculum of the first grade --that is, teachers concentrated on bringing up to scratch the non-experimental children, on those items of which the experimental children excelled. It may well be that first grade, or kindergarten, as the case may be, tends to be too similar to the intervention programs, and that it is only later that effects will show.

Now for some of the more intricate threads in the pattern. The first of these is what in Weikart's (1969) terms we might call the functionally equivalent curriculum. There are a number of curriculum comparisons going on these days; also, the Follow Through Programs and the Head Start Programs are moving in this direction. Probably we shall have data rather soon which will give us some light on this matter. But for now we might look first at the general conclusions Weikart draws from his own carefully planned venture into curriculum comparison. Weikart lists three areas which may be relevant. The first of these is the curriculum content. Each of his curriculum approaches had a clear commitment to a given theoretical model. The models were different, but each provided a general conceptual framework within which the teacher operated. The second item was the general planning and implementation of the program. Lesson plans were based upon the specific goals of the theoretical framework. In Weikart's words, "These plans have proved to be a daily struggle...providing opportunity for a constant review of curriculum effectiveness." Such a program inevitably demands great commitment on the part of the staff, and continual supervision. Weikart's final area is program operation, in which he includes the involvement of the mother, the specific focusing on the individual child, and the focusing on the child's educability. As he points out clearly, in all three curricula approaches there was a heavy emphasis on language. Karnes (1969) makes a similar suggestion.

Another relevant area I would include, although it is probably implied in the three that Weikart mentions, is the emphasis upon the motivational component in sustaining a gain. Changes in attitudes and interests relating to school activities may have major effects over time.

All so-called "good" programs of preschool intervention placing a heavy emphasis on language, on providing and stimulating interaction with a rich environment, and a high degree of individualization of the program for the given child may serve equally well to increase the child's general educability. This general stance is evocative of the earlier studies of psycho-therapy, where patients seem to get better as often with one approach as another, but at the same time experienced therapists have a better record of success than the inexperienced ones.

Weikart (1967) in an earlier paper, has also suggested another aspect of this matter of the nature of the curricula--that the curriculum most needed for the three-year-old may not be the best for the older child. The techniques generally used today may be better at promoting gain at earlier ages--they probably increase motivation, stimulate language, and in general get the child interacting with the environment and the adults in it. Once this is done more precise techniques may be needed.

I should like finally to come to one of the characteristics of the Weikart study and of our own--that of the nature of parent involvement. Weikart does not make a differential test of parent involvement, but the weekly home visits over the period of two years may well be one of the important factors in the size of gains that he has been able to demonstrate. The Early Training Project had about the same amount of parent involvement, but it was directed somewhat more directly toward bringing the parent into the situation as a teacher for her child than Weikart's was, at least according to his early reports. Merle Karnes' (1969) program has involved the parents in work in small groups. The important thing in all of these would seem to be not only the effect upon the children involved in the intervention, but the spillover effect on other children in the family. The research in which some of our group at Peabody have been engaged in the last three years has been designed actually to test comparisons of maximum involvement of mothers, a home visitor program for mothers, and a no-intervention for mothers but intervention for the target age child alone. The target age children in this study have just completed first grade this June. In general, our findings have been ones of being able to effect and maintain over time, beginning with an age of three, gains in intellectual ability. When the children meet in small groups for four hours a day for forty weeks or more the additional emphasis upon the parent does not seem to affect the performance of the target age child appreciably. Karnes' findings are similar. Marked differences occur, however, with the younger siblings of these children. Here we have found that with the mothers involved in the program the younger siblings are superior on intelligence tests and on a measure of concept development. It is worth pointing out, however, that what Weikart did, Karnes did, and what we have been

doing is a long way from typical parent education, which tends to be a fairly passive process of imparting information. The active involvement of the parent seems imperative.

And so we come to the end of this presentation, needing a conclusion. About the only conclusion we can make is that intervention programs can make a difference when carefully defined, implemented, and assessed. It is a long, difficult road, however. Anyone is going to be rudely disillusioned who thinks that there is one simple way--a button to press, or a lever to pull. Massive deprivation demands massive measures. And it demands these over time. The environment which created a deficit will continue to take its toll after intervention ceases, unless the environment is improved. For the next few decades early intervention probably is the necessary condition for improving the educability of disadvantaged children, but it is certainly not a sufficient condition. If the school, community, and home cannot work to sustain that early gain, it is no wonder children continue to fall behind as they move through their school years. We need better intervention programs, with improved techniques, and more appropriate targets, and ones with a more massive input. While some must work to improve the national scene in the immediate future, others must work to develop and test more effective programs, which over time can be put to wider use.

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